which would set at rest a question which, though I consider it settled in my own mind, is yet, I believe, held to be still doubtful by many interested in these matters.

In what I have written I have touched only upon obvious work suggested by the previous observations. I have little doubt that the preparations of the skilled astronomers of the United States include many surprises and daring attempts among the solid work which we are quite certain of.

All here wish them the extremest measure of success, which I am sure their efforts will do more than command.

J. NORMAN LOCKYER

ATLANTIC SHELLS

Testacea Atlantica; or, the Land and Freshwater Shells of the Azores, Madeiras, Salvages, Canaries, Cape Verdes, and Saint Helena. By T. Vernon Wollaston, M.A., F.L.S. Royal 8vo, pp. 588. (London: L. Reeve and Co., 1878.)

IT is with a saddened feeling we take up our pen to notice this valuable contribution to malacology; for ere its pages had left the hands of the binder, its talented author had passed "into the shadowy land."

The name of Wollaston is connected ancestrally with more than one department of science, and the author of the present work has well maintained the honourable reputation of Dr. Wollaston, the discoverer of palladium and rhodium, and the founder of the Wollaston Medal and Award.

Compelled in 1847 to visit Madeira on account of his health, he commenced to collect the land-shells of the various outlying islands and rocks of the Madeiran Group; and although (as he tells us) insects, rather than mollusca, formed at that time the main object of his researches, he was able to add a considerable number of unmistakably new species to the careful and elaborate catalogue which had previously been compiled by his friend and companion, the late Rev. R. T. Lowe, then chaplain at Funchal, Madeira, and to whom the present work is dedicated.

So interested did he become in the insects and land snails of Madeira, that, although no longer compelled to submit to exile on account of his health, yet he returned again and again to Madeira and spent many weeks under canvas high up among the mountains collecting.

In 1858 he visited the Canaries in the yacht of his friend, Mr. John Gray, and again in 1859. On both these expeditions he was accompanied by Mr. Lowe. He was thus enabled thoroughly to explore the numerous and widely-scattered islands of the Canarian group under the most fortunate circumstances for collecting.

Under the same happy auspices he visited the Cape Verdes in 1866, Mr. Lowe again being his companion. In 1875 Mr. Wollaston sailed for St. Helena with Mr. Gray, where he spent six months in investigating the natural history of that remote little oceanic rock, being on this occasion accompanied by Mrs. Wollaston; the Rev. R. T. Lowe, his friend of many past years, having lost his life in 1874 on his outward voyage to Madeira.

Mr. Wollaston has felt it desirable to place these facts on record, in order to show that the several islands and archipelagos treated of in the volume before us—with the exception of the Azores—had all been visited personally by himself.

Although this book contains descriptions of no fewer than 558 species and varieties of land and freshwater mollusca, the author does not claim for it the position of a monograph, but rather a critical enumeration of all the forms hitherto recorded, with special reference to *habitat* in the several Atlantic archipelagos.

Out of the large number of species and varieties described in this work, there are only twenty-nine which are claimed by the author as *actual novelties;* sixteen of these are from the Canaries, nine from Madeira, two from St. Helena, one from the Salvages, and one from the Cape Verdes. Mr. Wollaston would have conferred a still greater service on his fellow-workers had he given short diagnostic characters of all the species enumerated. This would greatly have facilitated the identification of the various forms and saved the student much time and avoided the necessity of referring in many instances to other works. It is also much to be regretted that references are not given to the excellent published figures of most of the species which are to be found in Reeve's "Conchologia Iconica" and the second edition of Martini and Chemnitz's "Conchylien Cabinet" by Küster. Well-drawn and correctly-coloured figures are almost indispensable for the accurate determination of land-shells where form and colour are dominant characters. It is easy to *see* and identify the form, when carefully delineated, but almost an impossibility to convey it to the mind in words.

Mr. Wollaston has shown throughout the strongest preference for the limitation of species—at times becoming extremely hypercritical—and in some instances he seems to be altogether in doubt as to what constitutes specific rank. For example, under *Helix bicarinata* (*vide* p. 161), he states that he is far from certain that it is more than a phasis of *H. echinulata;* yet a few lines below he observes that he has never found a single example among thousands which could be strictly regarded as intermediate.

Again (p. 209) *Pupa fanalensis,* "this may be only a depauperated state of the var. *β. anconostoma* of the *Pupa umbilicata,* which the latter has gradually assumed through having found its way into the higher regions, nevertheless I believe it to be truly distinct."

It is strange to find a man with Wollaston's admitted talents and vast opportunities for observation struggling hard against the accumulated evidence of more than thirty years, and clinging tenaciously to the last to the doctrine of the immutability of species. Thus in his Summary (p. 561), when commenting on the difficulties which arise in defining what is a "species" and what a "variety," he adds, "these remarks are by no means intended to insinuate that the lines of demarcation between species, when correctly interpreted, are ever, in my opinion, *really* confused or doubtful, the exact opposite having always been my firm belief."

Eliminating what Wollaston calls "the European or more distinctly Mediterranean forms" from the catalogue, so that only "the Atlantic element" remains, "the actual species which range beyond the limits of a single archipelago are marvellously few—about four or five being common to the Madeiras and Azores, about five or six to the Madeiras and Canaries, and about one to the Canaries and Cape Verdes; whilst between the Azores and Canaries there are only about five, and between the Madeiras and Cape Verdes about one. Moreover there are strong reasons for suspecting that some even of these (perhaps, indeed, most of them) may have been accidentally transported amongst the islands, through indirect human agencies, at a comparatively recent date; so that we are driven to conclude that, so far as the absolute *species* are concerned, of which their aboriginal faunas are respectively made up, the groups are practically almost independent of each other. And yet, in spite of this, I have had occasion to insist more than once upon the many characteristic *types* which, under the aspect of totally different but nevertheless allied species, permeate *to a greater or less extent* the entire 'province,' giving to it an amount of *unity*, through its several component parts, which it is scarcely possible not to recognise." (P. 563.)

The "*Atlantis* hypothesis" was clearly present to Wollaston when he wrote:—

> "There may doubtless be many explanations, perhaps equally plausible, of these phenomena, but I must confess that none commends itself so thoroughly to my mind as the possible breaking up of a land which was once more or less continuous, and which had been intercolonised along ridges and tracts (now lost beneath the ocean) which brought into comparatively intimate connection many of its parts, even whilst others, though topographically near at hand, were separated by channels which served practically to keep them very decidedly asunder. It is on some such principle as this that I would account for the Canaries appearing to be not only as widely removed from Madeiras as perhaps even the Cape Verdes are, but (whilst further to the south) *to possess a fauna of which the 'Mediterranean' element is much more traceable.* This latter circumstance, which is shadowed forth likewise by the Coleopterous statistics, is by no means a fanciful one, whole groups which are indicative (more or less) of Mediterranean countries, but which have no single representative elsewhere in these Sub-African archipelagos, being quite at home at the Canaries." (P. 565.)

An evolutionary phase of mind must have prevailed with Wollaston when he penned the following sentence:—

> "It is quite clear that the depression of certain tracts, and the upheaval of others, would produce an amount of disturbance in the fauna which could not fail to show itself *in some one way or other* which would afterwards become more or less decipherable; and I cannot conceive much difficulty in picturing the kind of change which might be brought about by the isolation of a cluster of individuals on a small rock, destined henceforth to become the habitat of a race which would, we may feel well nigh certain, rapidly mature for itself *some* slight distinguishing mark." (P. 566.)

But he quickly returns to his former state, and adds:—

> "Considering how unmistakable the evidence is for the variability (in this particular sense) of many of the Atlantic types—a 'variability' so decided that a slightly different phasis has been assumed in certain of the Archipelagos, for nearly every separate island and isolated rock, it may sound, perhaps, somewhat paradoxical to speak, nevertheless, of their apparent freedom from further change; and yet if there is one fact more distinctly shadowed forth than another it is, without doubt, their *present stability*." (P. 566.)

Further on he continues:—

> "After the most rigid and conscientious inquiry, I am bound to add that the 'developments,' so called, which might well be supposed to have been slowly elaborated, are (if any) simply *inappreciable*." (P. 567).

However widely we may disagree with Mr. Wollaston's conclusions on the *quæstio vexata* of species and varieties, his critical remarks are of great importance from the large series of specimens examined by him, and the fact that the types themselves were, in many cases, in his possession.

In an admirable lecture "On Insular Floras," by Sir Joseph Hooker, delivered before the British Association at Nottingham in 1868, he described the Madeiran flora as composed of two elements, the one clearly allied to that of the shores of the Mediterranean, the other totally different, and allied to none other but what is found in the Canaries and Azores, which he designated "the Atlantic Element."

That Sir Joseph Hooker's researches on the flora and Wollaston's observations on the insect and molluscan fauna of these Atlantic islands should bring out precisely similar results, will seem the less surprising when we remember the direct connection and interdependence existing between plants and insects, the latter acting as the great fertilising agents to the former; whilst the dependence of land snails upon plants is equally manifest.

All the difficulties raised by Wollaston as to the rare, peculiar, and isolated forms described by him were met and answered by Sir Joseph Hooker ten years ago. Assuming these minute islands to be relics of an older and once larger land-area which had been gradually reduced by subsidence, he pointed out that such a change, by contracting the area would intensify the struggle for existence. He showed that they were not new forms likely to increase and multiply, but rather old forms dying out. Also that in this exterminating process man was even a more destructive agent than the subsidences of land. For instance Madeira when discovered was so densely wooded that the settlers set fire to the forest and the fire raged for seven years, no doubt exterminating many species and reducing the number of others proportionately.

In Porto Santo rabbits had proved even more destructive than man; whilst in St. Helena, the introduction of goats in 1513, had almost exterminated the forests and the subsequent replanting of the island with exotic plants prevents the remaining indigenous vegetation from resuming its sway.

Whatever be the extent of area which we reclaim from ocean for our ancient "Atlantis," it is evident that formerly intercommunication existed between the Azores, Madeira, the Canaries, the Cape Verdes, and Southern Europe in Miocene times, for Prof. O. Heer considers some of the *Helices* of Porto Santo to agree with those of the Swiss molasse.

The poet's dream may therefore well be realised by the geologist:—

> "Which tells, great pictured Continent, of thee
> O blest ATLANTIS! can the legend be

Built on wild fancies which thy name surround?
Or doth the story of thy classic ground
With the stern facts of Nature's face agree?
What if no tongue may tell!—thy halo fair
Still lingers round the isles which slumber there."
("Lyra Devoniensis," p. 135).

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Indian Rainfall

As Dr. Hunter has been good enough to mention my name in his letter to NATURE (vol. xvii. p. 59) in connection with a comparison of the rainfall of Northern India and the sun-spot period, I may, I trust, be allowed to express my opinion regarding the validity of some of the conclusions he draws therefrom. In the first place I would remark that Dr. Hunter's idea of the *winter* rainfall of Northern India being due to the *immediate rebound of the summer monsoon* from the Himalayan barrier is at variance with facts in the meteorology of the country. The rebound ought to take place directly the monsoon vapour-current impinges upon the Himalaya, *i.e.*, in the *summer*. In fact, it is by a succession of oblique rebounds from this impassable barrier that the monsoon is gradually reflected towards the N.W.P. and the Punjab.

The *winter* rains, on the contrary, have nothing to do with the monsoon, being, as is well known, due to a branch of the anti-trade, which, descending in the Punjab, is deflected by the Himalayas towards Behar and Bengal, occasionally reaching Calcutta, lat. 22° 35′ N.

Now between the rains of the summer monsoon and those of the anti-trade (or anti-monsoon as it is occasionally called), there is a well-marked interval of bright, clear, settled weather for two or three months throughout Northern India. After this interval the skies again become clouded, and about Christmas, or shortly after, the gentle but soaking rain of the cold weather sets in, and is repeated at intervals up to the end of March. It is evident, therefore, that the two currents, monsoon, and anti-trade, are totally unconnected with each other; and hence arises the desirability, especially in a question like the present, in which its secular variations are being discussed, of completely separating the rain of the former from that of the latter vapour current. I cannot but think that it is his omission to allow for these vapour currents that has led Dr. Hunter to offer such an erroneous explanation of the results obtained. According to him, copious precipitation should take place in the interval (October to December) between the two seasonal falls, during which clear weather is invariably present. It may be added that the period (December to April) which I took to comprise the winter fall, commenced after this interval.

The real explanation of the direct variation of the summer, and the inverse variation of the winter rainfall of North India, with the sun-spot period, is the hypothesis which first led to its verification coincidently and independently, by Mr. Hill and myself.

To enter upon a complete exposition of this hypothesis would occupy too much of your valuable space, but as it has been found to explain most of the anomalies which have hitherto proved such powerful obstacles (especially in extra-tropical regions) to the universal extension of the theory of sun-spot influence (I use the term advisedly) on the different elements of terrestrial meteorology, I will here briefly indicate its general outlines for the benefit of other workers in the same field of investigation.

The hypothesis, to start with, assumes *the solar radiation to vary inversely with the sun-spot frequency.*

It then takes account of the probable effects of such a variation upon the vapour-bearing currents throughout the globe with respect to velocity, direction, season, and latitude. According as trade, anti-trade, monsoon, or anti-monsoon, prevail (1) at different places at the same season, (2) at the same place at different seasons, so will specifically distinct effects arise both from the amount of vapour brought and its conditions of precipitation, to determine which, not only the general conditions introduced by latitude and season, but the local and peculiar meteorological functions of the region must be carefully studied.

Now as the principal effect of a secular change in solar radiated heat must be to cause a similar direct secular change in the normal convection currents of the atmosphere, we may expect the tropical trade-wind and monsoon regions to furnish us with some evidence, whether direct or indirect, in favour of the above hypothesis.

Little direct evidence has at present been adduced besides that given by Mr. Hill from a comparison of wind velocity in the N.W.P. (NATURE, vol. xvii. p. 505). A good deal of indirect evidence, however, is furnished in the monsoon regions by the occurrence of abnormal droughts and floods in contiguous districts (the drought in the N.W.P. and floods in Assam and Burmah last year were good examples of this kind) at the time of minimum sun-spot, when the velocity of the current being increased it travels in a more contracted channel, and, by a more equable distribution of rain at the time of maximum sun-spot, when the velocity of the current being decreased, it is more liable to extend laterally. In the trade-wind regions similar evidence is furnished by the fact of a deficiency of rain and cyclones at the time of minimum sun-spot, with a corresponding excess of both at the time of maximum sun-spot. The augmented velocity of the wind currents at the former epoch, preventing the formation of local areas of condensation and precipitation, and therefore (according to Messrs. Blanford and Eliot's theory of cyclone generation) of cyclones and their accompanying downpours; while the diminished velocity at the latter epoch favours the same.

Finally, the anti-trade which in its seasonal shifts north and south traverses the entire temperate zone, in the winter bringing rain to North India, Palestine, Madeira, California, &c., and in the summer to Northern Europe and Siberia, should give signs of a secular change in intensity and humidity, corresponding according to the hypothesis inversely with the sun-spots. In the summer, when large continental areas like Europe are more immediately under the direct influence of solar heat, local convection currents being set up will tend to disturb and complicate the effect of any general change in the strength of the anti-trade. In the winter, on the other hand, the obliquity of the incidental solar rays leaves the anti-trade in undisputed possession of the field. At this season, therefore, there should be a marked variation in the rainfall of the temperate zone, more particularly in those regions between 25° and 40° N. and S. lat., where the rainfall of *this season* is the chief rainfall of the year, corresponding *inversely* with the sun-spots. Even in those regions where the rain falls at all seasons, if we pick out the winter from the total annual falls, as was done by Mr. Draper, for New York (NATURE, vol. xvi. p. 15) in accordance with Mr. Hill's admirable suggestion (vol. xvi. p. 505), the results favour the hypothesis. But they do this in a far more marked manner where the rain of the entire year falls during the winter months, as in the Mediterranean and at Jerusalem, which have consequently hitherto been considered by Dr. Jelinek and Mr. Meldrum to afford strong evidence against the theory of a direct connection between rainfall and sun-spots. The inverse variation of the winter rainfall of Northern India is only another example of the same law, and shows how extremely important it is to analyse the seasonal variations separately before deciding the question by a mere cursory glance at the *total annual* falls. The apparent anomalies which Dr. Hunter finds presented in the North American rainfalls are, I think, due to his having compared the total annual falls. If he and other investigators will only take the hint dropped by Mr. Hill, and which I cordially endorse, of comparing the seasonal falls separately, they will find, I think, that while the summer rainfalls of the temperate zone show either a non-periodic variation, or symptoms of one coinciding directly with the sun-spots, the *winter* falls will in general show unmistakable signs of a variation coinciding *inversely* with that of sun-spot frequency and area.

E. D. ARCHIBALD

Sun-spots and Rainfall

I HAVE read with much interest Dr. Meldrum's paper on Sun-spots and Rainfall in NATURE (vol. xvii. p. 448), particularly that part of it in which Dr. Hunter's method of discussing the rainfall of Madras is criticised, and a method of inquiry in sun-spot researches is proposed. This method is, so far as I am aware, a new one, and as such, is deserving of careful examina-